

Crime Prevention: A Cautionary Tale

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Those who design policies and those who do research frequently seem to speak different languages and often believe they seek different goals. Yet sound policies should build on good research, and sound research must take into account how social and physical contexts influence outcomes.

Fortunately, in criminology, policy makers and researchers can assume the shared goals of reducing crime and promoting justice. Therefore, both must be concerned with how crime-prevention policies affect clients of the programs they are designed to help.

The question of whether a practice reduces crime, however, is an empirical question. It is an empirical question about which there are few scientifically credible answers.

We would know more if policy makers understood criteria for good research and refused to accept the results of bad research. We would know more if researchers took into account the fact that policies are put into place with constraints that must be taken into account when recommendations are transformed to actions.

What I have to say today will be aimed at making three points.

First, that although we know a good deal about the predictors of crime, and though that information may provide reasonable hypotheses for practice, it is insufficient as a guide to knowing what programs will be effective in preventing crime.

Second, like some promising medicines, some social interventions have had harmful effects. Therefore, scientifically credible research to detect results of social treatments are as essential as they are for pharmaceutical treatments.

Third, the fact that a program works at one time and in one place is no

guarantee that it will work elsewhere and at a different time. Constant vigilance is a sensible stance.

Now for the evidence.

We know a good deal about the kinds of conditions that promote crime. These include knowledge that criminals tend to come from backgrounds in which they have been exposed to disorganized communities, to dysfunctional and neglectful families, to schools that fail to provide them with successful learning experiences, and to biological challenges that may include neurological abnormalities or nutritional deficits.

Many observers of these facts have believed that they could draw from them conclusions about appropriate ways to reduce crime. Let me tell you about one such project: The Cambridge-Somerville Youth Study.

Its creator, Richard Clark Cabot, laid the foundation for a scientific approach to studying intervention programs. Convinced that delinquency arose through general social deprivations, Dr. Cabot decided to evaluate the widespread belief that offering friendly guidance and social support, healthful activities after school, tutoring when necessary, and medical assistance as needed would reduce the likelihood of delinquency in impoverished areas. Perhaps even more importantly, Dr. Cabot insisted that the program be evaluated with a scientifically credible design.

Dr. Cabot and his staff collected the names of boys living in the congested urban environments of Cambridge and Somerville Massachusetts. The children were under the age of ten at the time of their referrals. To avoid having the program thought to be a program for bad kids, names were collected from scout leaders as well as the police. Although many of the boys were known as troublemakers, many were not.

Staff interviewed teachers and parents to learn about the youngsters and their families.

Psychologists and medical doctors tested the boys. Each boy was matched to another of similar age, social background, biological somatotype, and temperament. Toss of a coin determined which member of each matched pair would be placed into the treatment group and which into the control group.

Treatment began in 1939 and continued an average of 5.5 years. Case workers assigned to treatment boys visited them as frequently as weekly but the average frequency was twice a month. The counselors provided friendly guidance to the boys, counseled parents, assisted the families in a variety of ways, and referred the boys to specialists when that seemed advisable.

Because the logic of the study required being convinced that the treatment and control groups would have turned out similarly but for the introduction of treatment, it was important to know that despite randomization, no major differences had inadvertently crept into the study. Therefore, the groups were compared after a reduction of case loads due to wartime gas restrictions had taken place. No reliable differences were discovered in the comparisons.

The variables used for comparison give you an idea of the degree of care taken by the staff to assure integrity of the research design. The list also indicates what people then believed might be risk factors for poor social development.

No reliable differences were discovered in comparisons of age, IQ, whether referral to the Youth Study had been as "difficult" or "average," or the delinquency prediction scores assigned by the Selection Committee on the basis of the boys' family histories and home environments. No reliable differences appeared in comparisons regarding the boys' physical health as rated by the doctor after a medical examination, or in mental health, social adjustment, acceptance of authority, or social aggressiveness as reflected by teachers' descriptions of the boys. Nor were reliable differences found in ratings of adequacy of the home, disruption of the home, delinquency in the home, adequacy of discipline, standard of living, occupational status of the father, "social status level" of the elementary school attended by the boy (a measure based on the occupational levels of fathers whose children attended the school),

or quality of the neighborhood in which the boys resided.

To summarize: The matched pairs of boys resembled one another on the conditions that both then and now are thought to produce delinquency. These included whether the boys were from disorganized neighborhoods, experienced family dysfunction or disruption, misbehaved in school, had parents with criminal records, and whether or not they had been aggressive in childhood. Satisfied that the evaluation had a good foundation, treatment continued.

When the program terminated in 1945, over half the treatment boys had been tutored in academic subjects; over 100 received medical or psychiatric attention; almost half had been sent to summer camps; and most of the boys had participated with their counselors in such activities as swimming, visits to local athletic competitions, and woodwork in the project's shop. Boys in the treatment group were encouraged to join community youth programs. The boys and their parents called upon the social workers for help with such problems as illness and unemployment.

Meanwhile, boys in the control group had not been assisted through the program. They had received, of course, whatever assistance might have been offered in the community.

Evaluations of the Cambridge-Somerville Youth Study treatment boys at the close of their cases indicated that many of the boys who had been identified as maladjusted when they entered the program had made fairly good adjustments. Thus, according to what was then a typical measure of success, improvement from prediction, suggested effectiveness of the program.

To determine whether the improved adjustment should be attributed to treatment, however, interviewers tracked down 148 boys who had been in the control group. These boys, their families, and their school principals were interviewed. Dr. Helen Witmer, who had not previously worked with the project, classified each boy among the 148 pairs in terms of adjustment. Disconcertingly, the results indicated that almost equal numbers of the control and the treatment group did better than had been anticipated at the beginning of the project. (A more complete description of the program

and its early evaluation can be found in Powers and Witmer, 1951).

In 1948, the Massachusetts Department of Probation searched their records for appearances of any boy either in the treatment program or the control group. Disappointingly, the results showed that a slightly larger number of boys in the treatment group had been in court (96 vs 92) and that they had been brought there for a slightly larger number of offenses (264 vs. 218).

Gordon Allport, President of the Board of Directors for the Ella Lyman Cabot Foundation, called for patience. He believed that change might be slow and that the benefits from treatment might appear as the youth matured.

Between 1975 and 1981, when the boys were reaching middle age, I retraced the 253 matched pairs who had remained in the program after the first cut in 1942. Questionnaires sent to men from the treatment group asked how, if at all, the program had helped them. Two thirds of the respondents listed ways they perceived it to have been beneficial. Some noted that it had put them on the right track. Others mentioned the friendships encouraged or the talents acquired. With these subjective endorsements in hand, we sought objective evidence of the program's effects.

We tracked court records both in Massachusetts and in the states to which the men had migrated. We tracked mental hospital records and records from facilities for treatment of alcoholism. We obtained death records to confirm deaths when this was reported or if a man had not been found.

The results showed that as compared with members of the control group, those who had been in the treatment program were more likely to have been convicted for crimes indexed by the Federal Bureau of Investigation as serious street crimes; they had died an average of five years younger; and they were more likely to have received a medical diagnosis as alcoholic, schizophrenic, or manic-depressive (McCord, 1978, 1981, 1992).

Indications that the treatment program, rather than an unmeasured difference between the samples, had affected the outcome of those in the treatment group

comes from two comparisons: First, the differences in outcomes favoring the control boys occurred only among those pairs in which the treatment families had cooperated with the Cambridge-Somerville Youth Study staff. Boys from the control group and those from the treatment group whose families were uncooperative were equally likely to do badly and to do well.

Second, there appeared to be a dose-response from both intensity and length of treatment. Boys whose counselors more frequently visited them, and those in the treatment program the longest were most likely to fare badly as compared with their matched mates in the control group.

To assess effects of the various treatment approaches, I computed an adverse odds ratio by dividing the number of pairs in which the treatment boy did worse than his match by the number of pairs in which the treatment boy did better than his match for each of the major emphases of the treatment program. Adverse odds ratios less than 1 would indicate benefits of the treatment program.

The odds ratio for bad outcomes for a focus on encouraging the boy to participate in group activities such as boy scouts and YMCA was 1.75 (35/20); that for a focus on providing academic help was 1.91 (42/22); that for a focus on personal problems, 3.5 (28/8); and 3.75 (30/8) for a focus on family problems. Although there were differences, with emphasis on problems seeming worse than those on activities, the differences were not dramatic. None showed benefits from treatment.

Had there been no control group, evaluators might have concluded that the program was beneficial because so many of the treatment boys were better adjusted than anticipated. Or, because two-thirds reported beneficial effects for themselves, evaluators might have judged that the program was effective. But these judgments would have been contrary to objective evidence that the program resulted in adverse outcomes for many of the participants.

The study is not alone in discovering, through carefully designed research, that sensible hunches and adequate implementation may produce interventions that fail to achieve their beneficial goals. Let me tell you about some of them.

Attaching volunteer counselors to juvenile courts has long been a favored practice in the United States. These programs are based on an assumption that young delinquents benefit from guidance by mature adults who can serve as their sponsors. Martin Gold arranged to evaluate a program that had already won community respect. His evaluation involved randomly selecting consenting juveniles to a program in which the volunteers provided group counseling, individual counseling, and tutoring. The control group received ordinary services of the court.

A year later, evaluators assessed the results. Both those assigned to the control group and those who had been assigned to the volunteer program but had not participated in it decreased their rates of crime. Those who participated in the volunteer program, however, increased their criminal activities - as measured both through official records and by self-reports (Berger, Crowley, Gold, Gray, & Arnold, 1975).

Two types of peer counseling programs have shown similarly adverse effects. Guided Group Interaction focused on building social skills and increasing self-esteem by providing troublesome youngsters with opportunities to discuss a variety of issues with well-adjusted peers. The programs became popular prior to any evaluation of results. Gary Gottfredson finally negotiated an evaluation in the schools of Chicago. The evaluation included random assignment of students in public elementary and high schools to either a treatment or a control group. Subsequent follow-up indicated no effects for the elementary schools and some detrimental effects for the high school program (Gottfredson, 1987).

A somewhat different approach toward training young adolescents to have increased social skills has backfired in a program administered by the Oregon Social Learning Center. There, aggressive youngsters were randomly assigned to one of four groups: a teen training group that encouraged self-regulation and socialized behavior, a parental training group that encouraged parents to track their youngsters' behavior and to praise them for positive deeds, both, or one in which tapes and booklets substituted for group interaction. Whereas the parental training group (without peer training) seemed to show benefits, both groups assigned to peer

training turned out worse than the no-interaction controls (Dishion & Andrews, 1995).

Other studies, too, have seemed to show negative effects. The studies reported above, however, are among the most carefully designed from the point of view of evaluation.

Negative effects have been found from using detached social workers to guide gangs into socially acceptable behavior. Klein (1971) discovered that the greater the success of detached workers in organizing a group of boys, the greater the increase in criminal activity of that group of boys.

Adverse effects from psychotherapy with aggressive youngsters have been found for patients treated at a child guidance clinic in St. Louis (Cass and Thomas, 1979). Similarly, Gersten, Langner, and Simcha-Fagan (1979) found adverse effects among six to eighteen year old children with a variety of psychological symptoms in New York City. These studies used statistical controls and are therefore less credible than studies based on random assignment to a treated and a comparison group.

Evidence, too, suggests that probation with neither fines nor required restitution is less effective than sentences that include real consequences for the offender (Glaser & Gordon, 1990; McCord, 1985). These studies, too, should be considered only suggestive because they were not based on randomly assigned eligible delinquents.

Although the possibility of isolating effects from the Cambridge-Somerville Youth Study had appeared intractable for several years, recently it seemed worthwhile to attempt another try. This time, the hypothesis was drawn from the theory of intentional action on which I have been working for several years (McCord, 1997, 1999, 2000). According to Construct Theory, actions are motivated by descriptions that become potentiating reasons when they provide grounds for behavior. A variety of influences can increase the probability that particular descriptions will play the role of potentiating reasons for actions. These include situations that increase status of or provide benefits for the actor. In a group setting, responses by peers can influence what will become a potentiating reason.

Treatment in the Cambridge-Somerville Youth Study had specifically included summer camp. The camps selected for placement were not designed for troublesome kids. They catered to a general population, one for which summer camping offered an alternative to city heat and boredom as well as the pleasures of outdoor activities.

Among the 253 matched pairs assessed for follow-up, 125 of the treatment boys had been sent to summer camp. These boys were in a position of close association with peers, many of whom would be likely to be impressed by tales of daring. The fact that some children were sent to summer camps for several summers permitted further identification of possible mechanisms of influence. If the mere association with peers either increased or decreased the likelihood of an effect from intervention, summer camp might be expected to affect outcomes regardless of the frequency of attendance. If, on the other hand, camp influenced outcome through providing a forum that encouraged bragging about deviance, one would expect to find differences occurring particularly among those sent to camp more than once. After the first summer, these boys would have known what camp was like and be in a position to estimate the effects of their reported daring (whether or not these reports were factual).

Among the 128 boys never sent to summer camp, 25 turned out better and 28 turned out worse than their matched pairs. Among the 59 boys sent to summer camp once, 12 turned out better and 16 turned out worse than their matched pairs. Among the 66 boys sent to summer camp at least twice, 2 turned out better and 20 turned out worse than their matched pairs. The risk ratio for boys attending at least two summers of camp was ten to one!

Attendance at camp for at least two summers, though not for just one, differentiated those for whom treatment was harmful from those for whom it was benign. Still, there was no evidence for a beneficial effect of treatment.

The Cambridge-Somerville Youth Study was in many ways a model intervention program. The project was funded well enough to provide assistance over an average of more than five years. The design incorporated boys from good families as well as those at

high risk, thus avoiding stigmatizing participants. Treatment was individualized. The project was designed with matched pairs of boys, using random selection for treatment. And the records were complete enough to enable location of 98% of the subjects thirty years after treatment ended.

The results show that the treatment program was not beneficial. Indeed, a fair reading of the evidence suggests that parts of it were harmful. A good part of the harm appears to have come through something that happened at summer camp when the boys returned for at least a second stay.

We are a long way from understanding how this result came about. I strongly suspect that the boys from the Youth Study tended to bond together, encouraging one another's deviant values in much the style that deviant parents encourage their sons' deviance when they ask for reports about fights in school or other types of misbehavior (Bandura & Walters, 1959; Dishion & Poe, 1993). If the Construct Theory is correct, the encouragement is effective because peer responses among misbehaving children provide potentiating reasons for additional misbehavior.

The Construct Theory provides a plausible explanation also for the failure of a program popularly known as "Scared Straight." Inmates designed the program on an assumption that delinquency could be prevented by giving wild youngsters a taste of what it would be like to be imprisoned. The project started in Rahway Prison in New Jersey, where its endorsement by judges helped to make a film popularizing the program convincing.

Without scientifically respectable evaluations, "Scared Straight" projects were adopted in 38 states. There were congressional hearings about the program because researchers were skeptical. Miller and Hoelter (1979) found the town from which 13 of 17 youngsters in the film had come. They learned that some kids claimed to have committed crimes to prove they weren't scared.

Finally careful research was carried out, with random assignment to San Quentin's Squires Program or to a control group. Twelve months later, 81% of the experimental group and 67% of the control group had been arrested (Lewis, 1983).

Other scientifically credible evaluations, too, have shown that attempts to scare teenagers into better behavior is not a successful enterprise (Petrosino, Turpin-Petrosino, & Buehler, 2001).

Clearly, some popular interventions have harmful effects. Such is not the case for other intervention programs. I've been associated with the evaluation of two successful programs. But today, I do not have time to describe them. Rather, let me emphasize the importance of using objective, scientifically credible evidence to evaluate the programs we use in attempting to improve society.

We know, thanks to the careful work of a handful of researchers, that social programs can have enduring effects.

We know too, that well designed and carefully executed programs can have harmful effects.

A case can be made, therefore, that adequate, credible evaluations must be part of the normal operation of intervention programs. These evaluations should include a check for evidence of adverse side effects as well as benefits.

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